DISCOVERY PROGRAM ORAL HISTORY PROJECT
EDITED ORAL HISTORY TRANSCRIPT

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MESSENGER PRINCIPAL INVESTIGATOR
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40TH LUNAR AND PLANETARY SCIENCE CONFERENCE
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NIEBUR: This is an interview with Sean Solomon, the PI [principal investigator] of MESSENGER, which stands for the Mercury Surface, Space Environment, Geochemistry, and Ranging mission. We are at the 40th LPSC [Lunar and Planetary Science Conference] in Houston, Texas, on March 22, 2009. I am Susan Niebur, and I will be doing the interview. I am just going to say this again for the record. Thank you for signing the oral history release form. A copy of this audio recording and the transcript will be placed in the NASA Archives at the conclusion of this project. You will be given an opportunity to edit or to add to the transcript if there is anything that you want to change or need to add.

SOLOMON: No, that's fine. My experience with transcripts is that they're an imperfect rendering of the oral record, and anything I say orally I am sure can have its grammar improved.

NIEBUR: That is certainly fine. It is not a big deal. I want to be sure we get the history. So, one of the reasons I am doing this is because I wanted to understand more of what has happened on the mission from the PI's perspective and maybe PM's [project manager] perspective and really go back and see what we can learn from these missions. We have had so many already. So that is what we are looking for here. When we are all done, I will be doing some analysis and putting it together into a history of the Discovery Program. Hopefully, we'll make this available to

people who are thinking about planetary science or who are just interested in how this all works. So before we talk about the mission itself, I am going to start with a little bit about you. I don't think you have done an oral history for NASA before.

SOLOMON: No I haven't.

NIEBUR: Okay. So I would like to get some on the record. I know you started your career at MIT [Massachusetts Institute of Technology, Cambridge], and you were there for quite a long time actually.

SOLOMON: You mean my post-Ph.D. career. Yes, I joined the MIT faculty 10 months after my Ph.D., also at MIT, and taught in the Earth and Planetary Sciences Department, which became the Earth, Atmospheric, and Planetary Sciences Department, for a little more than 20 years.

NIEBUR: And I saw that from almost the very beginning. I could tell that you'd had a lot of interest in lunar and planetary. How did you get interested in that?

SOLOMON: Well, actually it goes back to my undergraduate days. I was an undergraduate at Caltech [California Institute of Technology] from 1962 to 1966, and I worked on campus for three summers in Earth science. I was a seismologist. JPL [Jet Propulsion Laboratory, Pasadena, California] was flying Mariners [spacecraft]. I vividly remember Mariner 4, which flew by Mars in July of 1965. There was a video system on campus where they would play back line by line the images from Mariner 4. There weren't that many images, but they were the first images from

Mars. And they were playing down to a TV there on campus. Whenever I passed that monitor, I stopped and watched these images come down. Even then, the division at Caltech [that was the home of my major] was evolving from a strictly geological sciences division to one that they renamed to Geological and Planetary Sciences. Bruce [C.]Murray had come out to teach classes, and some of the other faculty were doing classes that broadened from Earth science to planetary sciences. I was getting that exposure as an undergraduate.

So when I went to grad school, I went to MIT. NASA was investing in research groups that would work on lunar problems in anticipation of the Apollo landings, and there were groups working on lunar geophysics. They recruited grad students. So even though I wasn't a card-carrying planetary scientist I did work on planetary problems as a grad student. It wasn't part of my thesis, but it was part of my research experience.

NIEBUR: Who did you work for?

SOLOMON: My thesis advisor was Nafi Toksöz. He was involved in the Apollo seismic experiment. I don't know how he managed this, but the seismic team on Apollo divided the scientific questions that they would address, and Toksöz was given the task of determining the seismic structure of the Moon. He was fairly junior faculty. This was the first time that any cosmic body other than the Earth would have its seismic structure worked out, and he led that effort. So it was a really, really fun time. Originally Frank Press, who was the chairman of the department then, was also involved at a lower level, and others. Gary [V.] Latham down at Columbia [University, New York] was leading the effort overall and also the effort on characterizing seismic sources. That was a really steep learning time.

You probably know that the first seismometer was taken to the Moon on Apollo 11, but it didn't have a power source to last very long. It didn't last until Apollo 12, for instance. And it had all these crazy long-lived signals that nobody understood because they didn't look anything like Earth seismograms, and so the seismic team debated what they were seeing. They had all these explanations, none of which were right. Then on Apollo 12, it was the first of the ALSEP [Apollo Lunar Surface Experiments Package] packages that had the capability to last for years instead of just a few months. The astronauts set up the package and left it on the Moon, and then they used the Lunar Module as a seismic source. They crashed it back into the Moon. And only then did they see what a lunar seismogram looks like because they knew for the first time what the source was, and where it was, and when it went off. They realized that all of these wiggles they had seen on Apollo 11 were moonquakes and meteorite impacts that were so different from anything a terrestrial seismologist had experienced that they didn't recognize it until they knew what they had on Apollo 12. Then the whole lunar seismic network got built up. So that was a fun time.

NIEBUR: I bet. Then you became involved with Magellan?

SOLOMON: That was the next mission. That was the first space mission in which I was a co-investigator.

NIEBUR: How did you get involved in that? For your very first time it is often difficult to get involved. Magellan was a big deal.

SOLOMON: Magellan was a big deal. Let's see if I can piece all the threads together. I had worked off and on on planetary problems starting with the Moon. I got very interested in Mercury after Mariner 10 and wrote some papers on Mercury. I started getting interested in Mars after the Viking program and I wrote some papers on Mars. But all the time I was an Earth scientist as well and most of my research was in Earth seismology. But I maintained an interest in all of the inner planets.

I found myself, as Voyager moved out from the inner planets for the first time with a really capable payload to the outer planets, toying with the idea of getting interested in the outer solar system and decided I would be stretched too thin, so I stopped. But one of the reasons I got involved with Magellan was clearly the presence of Gordon [H.] Pettengill on my faculty because he led the winning proposal to operate the radar system on the original mission. You may know there was a Venus Orbital Imaging Radar [VOIR] that had a payload that was broader than just the radar, and it had atmospheric sounding, several other payload instruments, and a distinct altimeter. It was a cancelled for a time and re-scoped by JPL and reemerged as the Venus Radar Mapper, which was finally given a better name as Magellan. At that point it became a one-experiment mission, which is the radar instrument. It operated in both the altimeter mode and a synthetic aperture, side-looking radar mode. So that plus radio science was that mission. I got involved in Gordon's proposal, but I'd already been involved in the science definition team, I was already a member of [the National Research Council (NRC)] COMPLEX [Committee on Planetary and Lunar Exploration] at that point.

NIEBUR: Oh, back then even?

SOLOMON: Back then. So I was getting more and more involved in developing strategies for solar system exploration and getting asked to be on the committees that planned missions.

NIEBUR: That's fantastic. You must have been quite young at that time. That was 30 years ago.

SOLOMON: Yeah, that was even more than 30 years ago and I'm more than 40 now.

NIEBUR: That's pretty good. I didn't know there were teenagers on the mission (laughter).

Then you went on to do other Mars work?

SOLOMON: Yes, I was recruited by Dave [David E.] Smith to get involved in altimeters. Now I knew Dave Smith through my Earth science [work] because Dave ran a big effort centered at Goddard [Space Flight Center, Greenbelt, Maryland] called the Crustal Dynamics Project that integrated results from very long baseline interferometry and satellite laser ranging. This was right before GPS, to carry out research on crustal dynamics, on motions of the plates, on deformation in fault zones, [and] the response of the Earth to glacial unloading and ocean loading.

NASA built up a very well-run program centered at Goddard with research grants out to university investigators who would gather for an annual meeting. They made a great deal of progress. Dave Smith and some of his compatriots were managing that program. Dave and a couple of other people at Goddard at the time got interested in expanding into planetary science, in particular altimetry. Originally radar altimetry, not laser altimetry; that changed, too. They wanted some planetary credibility to their proposal. They came to me and said "Hey, you're a

planetary scientist. We know you because you're in the Crustal Dynamics Project. Would you go in with us on this proposal? There's this mission called Mars Observer. We want to propose an altimeter."

NIEBUR: Very cool.

SOLOMON: So that is how I got involved. As you may know, Mars Observer had some budget problems. The altimeter almost got tossed off. It came back as a laser altimeter because—

NIEBUR: Because the advances had been made?

SOLOMON: Advances had been made, and the laser altimeter could come in under the cost cap that a radar altimeter could not. But that set the path for that whole group thereafter.

NIEBUR: Right.

SOLOMON: Now Mars Observer, obviously, didn't work. There was a problem with the rest of the spacecraft, getting into orbit. But then that became the MGS [Mars Global Surveyor] MOLA [Mars Orbiter Laser Altimeter] instrument.

NIEBUR: Right. And that team has been very successful since getting so many new instruments.

SOLOMON: MOLA was a fantastic instrument. If you look at how that changed our

understanding of Mars, and everybody now just routinely uses the topographic data, it's just a beautiful experiment.

NIEBUR: That's wonderful. So when it came time to planning a mission of your own you were no stranger to missions, mission planning, science working groups, and things like that?

SOLOMON: That's right. But I had never been a PI even on an instrument on a spacecraft.

NIEBUR: I seem to recall that from our discussions in the past.

SOLOMON: I was definitely what a former Associate Administrator for Space Science would have called a rookie PI.

NIEBUR: That was the context in which we had that discussion. It was an interesting time. So when you thought about proposing a mission, what led you to first of all get the idea to go yourself? Tell me why you decided to propose the mission and why you chose Mercury.

SOLOMON: It actually didn't work the way you asked the first question. I knew about the Discovery Program. I hadn't proposed to it the first couple of rounds. You know I knew about the San Juan Capistrano workshop early on. I certainly knew about the early missions, NEAR [Near Earth Asteroid Rendezvous], [Mars] Pathfinder (which wasn't a pathfinder to anything).

NIEBUR: Well you'd been on the science definition team too so you had a history with that mission.

SOLOMON: Yes, yes. I knew that once it was the pathfinder, then it wasn't. And we shouldn't go there either. I was unhappy the way that worked out.

NIEBUR: A lot of people were, actually.

SOLOMON: Some of my colleagues like George [W.] Wetherill were on the review panel for Stardust. I'd learned from conversations during the early part of the program. But the way I got introduced to Discovery was to be recruited by APL.

NIEBUR: Oh really? So they reached out to you?

SOLOMON: APL decided that they thought [they could do] a Mercury orbiter mission. They were doing NEAR at the time. They had ambitions to do more things in solar system exploration. As you know, APL, before NEAR, had a very good record in Earth orbital missions, but primarily for military sponsors, not so much for NASA.

NIEBUR: Well they had some good luck with ACE [Advanced Composition Explorer].

SOLOMON: ACE came later. But yes. Tom [Stamatios] Krimigis was the head and had ambitions to do more. It was the year of faster, cheaper, better.

NIEBUR: Right.

SOLOMON: APL was a very lean organization with not a lot of hierarchical structure and fit very well into developing low-cost but technically ambitious missions, and [they] were always looking for good ideas. So if you talked to Tom Krimigis, Andy [Andrew F.] Cheng, or Ralph [L.] McNutt you would be able to have a conversation with people who were involved with this Mercury orbiter concept even before I was, because what they did then, APL, was to start recruiting a science team. The very first people they went to were the people they knew from NEAR.

NIEBUR: That explains why there is so much overlap between Co-Is [of MESSENGER and NEAR].

SOLOMON: Yes. Completely explains it.

NIEBUR: Fascinating.

SOLOMON: Eventually I got a call from John Appleby, who was the head of development for the space department at the time. He said, "We are looking to put together a team of scientists for a Discovery proposal, a Mercury orbiter. Would you be interested?" And I guess they called me because I had written some papers on Mercury, although most of them were pretty old. But I found Mercury pretty interesting after Mariner 10 and I even was one of the two authors of the

COMPLEX chapter on Mercury in the 1978 COMPLEX report on exploration of the inner planets.

NIEBUR: Really? Wow, okay.

SOLOMON: Back when I was 10 years old.

NIEBUR: Well, that predated the mission by 20 years.

SOLOMON: Yes, I know. [The other] was Gene [Eugene H.] Levy, whom you may know, who was at [University of] Arizona for many years, and a Dean, and is now the provost of Rice [University, Houston, Texas]. But he and I were the two youngest members of COMPLEX. Jerry [Gerald J.] Wasserburg was the chair, and he said "We need somebody to write the strategy for Mercury. You two guys."

NIEBUR: Like you've got time?

SOLOMON: Sure, that's what he was thinking. You want somebody that is in it for the long haul. So Gene and I wrote that chapter in the 1978 COMPLEX report. "What are the strategic goals for the exploration of Mercury after Mariner 10?"

NIEBUR: Wow. Okay.

SOLOMON: Now this is little known. There is nobody at APL who knew that. But I knew it.

NIEBUR: But it dovetailed nicely.

SOLOMON: So I was asked, "Do you want to be part of the science team for a Mercury orbiter?" I said sure. MGS was still going. Magellan had ended two years before, [and this] sounded like fun. I hadn't given a great deal of thought to Mercury for almost 20 years, then. This is 1996. Spring of 1996. But it was something I had wanted to do for 20 years. It was a chance. Of course I didn't know that much about APL or this team. And then John Appleby filed that information away, and the next thing I knew I got a call from Tom Krimigis.

I was out at JPL for a roadmap, some planetary exploration roadmap, meeting. And I called Krimigis back, and he said, "Can you come out to APL?" I had never been to APL. So I drove out there and I was late because I didn't know how bad the beltway traffic would be. I came into this room with 10 people waiting for me, and the gist of it was, they asked me to be PI. "Would you like to be PI? You already said you would be on the science team for the mission, how about being PI?"

Now, I don't know all of the thinking that went into it. I do know that in the previous round of Discovery, APL had not gotten any of their missions very far forward. And one of the messages that Krimigis had been handed from Headquarters was that in that round all of the mission concepts from APL had APL PIs. And he was told, "Look, you are going to do better if you get an outside PI. Because then you've got a much broader menu to choose from, and you could pick, if you could persuade them, any expert you want."

NIEBUR: And did they give you that flexibility to pick additional members of the science team, or had a lot of it already been formed?

SOLOMON: Yes. A lot of it had been formed. So when I came, there was already some agreement, and most of these were the NEAR people. "These guys have already agreed to be on your science team."

I said, "Well, that's okay, but I want to add. There are some people that I think this team needs that you don't have." And they said, that's okay. But I was naive in a lot of ways. So I don't know how many questions you want to ask about this, but that's how I got involved.

NIEBUR: Now tell me, go on. I am happy to listen. I've got some questions, but I'd really like to hear how things changed or if things you'd wished you'd known. How you felt, maybe you were naive at the proposal. It seems to be a very daunting task. I can't imagine, knowing what I know now, how anyone takes it on.

SOLOMON: I didn't appreciate all of the aspects of the things I would have to know. So, for instance, when we wrote that first proposal. The first time we wrote it, it got accepted to the second round.

NIEBUR: Right. To the concept study.

SOLOMON: I put a lot of effort into the science rationale, which was the first 25 pages of the proposal. But I had to accept that the engineering team really knew what they were doing. As

much as I could learn about engineering wasn't enough to be in a position to critically evaluate the confidence with which they had solutions to particular technical challenges. I didn't know that much then about risk management. I didn't know to ask all of the questions that I learned how to ask about. Okay, what is your contingency plan in case it doesn't work as you say it's going to work, since it's untested. Nor did I know how to evaluate project managers, the first time around.

NIEBUR: Which became important for your mission.

SOLOMON: That was very important for our mission.

NIEBUR: And many others.

SOLOMON: We had a dynamite scientific component to the proposal. That was easy because there has never been a spacecraft in orbit around Mercury. So you can do all the things that you can carry on your payload, and you'll do new things.

NIEBUR: It's all new.

SOLOMON: So that part was kind of easy. We could do a bang-up whole-planet study for the first time from orbit. But it had a thermal exposure of the equivalent of 11 suns. So the thermal design was absolutely critical. And anything that was exposed to the Sun was absolutely critical. And we needed a really good management team to be able to carry this out. So when we got to

the second round, which is all about TMCO [Technical, Management, Cost, and Other factors review], not about science, then I was a rookie PI, and some of the things didn't come together as quickly as they should have.

At the time of our site visit, we had a development path for the solar arrays, which was worked out, but in the questions and answers it was clear we didn't have a sufficient contingency plan. If any of the testing proved that our assumptions were not appropriate, the solution that was envisioned didn't seem to work in the testing; we didn't have a deep plan for what to do next. And so we were really sharply dinged on the solar arrays, which have to face the Sun.

NIEBUR: That was such a challenge. Did you start with the original design? Was the original design similar to what you ended up with, where you alternated the mirrors and the cells?

SOLOMON: Yes.

NIEBUR: Because that is really ingenious. Had that been used before?

SOLOMON: That I don't know. The two big challenges for the solar arrays, the junctions—they had to survive at high temperatures—and the way the solar cells are attached to the panels and substrates and adhesives.

NIEBUR: And what did you know about adhesives?

SOLOMON: Nothing, nothing, nothing, nothing. But we hadn't done enough testing to be absolutely confident to the level of being able to persuade a legitimately skeptical review panel that we had the right solution. When it came to alternatives to the path we had chosen, that had not been well thought out. And the other place we got hammered was that the budget did not come together. This was the project manager's fault. It didn't come together in a way that could be shared with the team, including the PI, before the site visit. The budget was so late that he didn't put all the numbers together until the night before the presentation, and some of that information that had gone out to the site review team didn't add up. The numbers didn't add up.

NIEBUR: That wasn't the last time it happened, was it?

SOLOMON: It was the last time it happened on MESSENGER.

NIEBUR: Okay.

SOLOMON: But he was in front of the room and getting questioned on budgets, and he couldn't answer the questions. And there was nobody there who could help him because nobody had seen it. It had been put together so last minute.

NIEBUR: Were the science and the engineering management really parallel tracks at that point? I have always wondered about how successful teams properly integrate. Clearly, you didn't have complete control over that at all.

SOLOMON: No, no, I didn't. And I didn't know enough to be more insistent on seeing the budget side, on grilling the engineers on how they would deal with risk on the most risk-prone parts of the payload. So I went in with a lot of trust in the engineering team, built in large part on the fact that most of the engineers involved on this effort were really smart guys. And women. I came to have a lot of respect for APL as a pool of talent and as a group that was able to work together. A lot of these people had worked together on NEAR, so they all kind of knew each other.

We thought there was more heritage than there turned out to be from NEAR on a lot of subsystems. So it all seemed to make sense. People had a lot of confidence. I wasn't sufficiently skeptical in the areas where I was ignorant. So I certainly bear a lot of responsibility. But when it came to the site visit, those were by far the two weakest parts of our presentation, the solar arrays and—well the way Jay [T.] Bergstralh phrased it when he kept reading the same answer to me despite me asking more questions when we got the debrief. He said, "The solar arrays and coupled management issues."

I said, "Coupled management issues, you mean the budget didn't add up?"

He said, "The solar arrays and coupled management issues."

NIEBUR: What it unclear to you what he meant?

SOLOMON: I was looking for a little more feedback for the next time.

NIEBUR: And the instructions at the time, I believe the instructions are probably still the same, that you are only allowed to read the results from the TMCO review.

SOLOMON: I think they now give a written version.

NIEBUR: They do give a written version.

SOLOMON: We did not get that.

NIEBUR: Yes, that was just in the last one.

SOLOMON: We certainly knew after the site visit where we had been the weakest. Because it had been apparent with the questions and answers.

NIEBUR: And then did you and APL immediately then go improving it for the next round since you had done so well in that first round?

SOLOMON: Pretty much. We had a meeting and agreed that we would re-propose. I said I want a new project manager.

NIEBUR: Oh. So that wasn't—

SOLOMON: That was the condition of my being the PI on the next round. I got to say who my project manager was, and it wasn't going to be that one.

NIEBUR: Did you get to select the person, or did you have approval over who was nominated?

SOLOMON: I had approval. I had approval. They did not have so deep a stable of project managers that I could sit in front of a one-way mirror and have a line-up of project managers go across the stage. They said, "Okay, you get to approve project manager."

NIEBUR: As a PI coming from academia, or a research lab more or less [the Carnegie Institution of Washington], how do you even know who to choose as a project manager?

SOLOMON: I knew a lot more then than I did the first time.

NIEBUR: Okay. It was the feedback?

SOLOMON: I knew what skills I wanted, including communication. That we had to have a rapport, someone who could work well with his own engineers. Somebody whose budgets I believed. Somebody who knew about risk. Somebody who had had some experience under the belt. They said we think we have somebody for you. We would like you to meet Max Peterson. Max and I hit it off. So he became the proposal manager and the project manager for the second [proposal] time. So that solved our first problem, which was the "coupled project management issues." We had to solve the solar array problem. And APL did that by doing the testing.

NIEBUR: Excellent. Just on their own dime?

SOLOMON: Their own money. They developed a testing protocol. They put the resources in. They figured out how to do the test at NASA Glenn [Research Center, Cleveland, Ohio]. So by the time we wrote our second proposal, and particularly by the time of the second site study, we could say, "Not only do we have a solution for the solar arrays, here are all the tests that validate our models."

NIEBUR: That must have blown them away.

SOLOMON: Yes, they loved it. So, the first time we proposed we were low risk in round one and high risk after the site visit, high risk being the solar arrays and not having a good project manager. But we were low risk both times the second time through.

NIEBUR: Excellent. On that second time through, you were selected.

SOLOMON: We were.

NIEBUR: Now one more personnel question. I noticed that MESSENGER was one of the few missions that had a project scientist in addition to a principal investigator. In the Discovery class of missions there really have only been 3 project scientists.

SOLOMON: I did not know that.

NIEBUR: How did you see that relationship working?

SOLOMON: I wanted a project scientist who would spend at least part of every day working the mission face-to-face with the engineers.

NIEBUR: Co-located?

SOLOMON: Co-located. As it happened, my ability to collaborate with APL was very much enhanced by my proximity. I didn't have to get on an airplane to go to APL. It's a half-hour drive. So I could go out there once a week. It wasn't a strain on my travel schedule.

NIEBUR: Did you find that was something that became important as you went through implementation?

SOLOMON: Yes, very much so. I don't know how I would have done that if my implementing organization were on the other coast. Because I couldn't develop that kind of frequency of on-site visits. There was no way I was going to be able to have daily exchange as needed with all of the subsystem engineers who were working on this. But a project scientist who was at the implementing organization, at APL, could do it. And from the start it was Ralph McNutt. He and I had a good ability to communicate. We thought similarly on a wide variety of issues. And so it was a nice team.

NIEBUR: Excellent. Now, tell me more about the teaming. Tell me about your science team. How did they work together?

SOLOMON: How did they work together? Now, looking backwards, from the perspective of 1996 or from the perspective of 2009?

NIEBUR: Oh, whichever is more interesting.

SOLOMON: Well, it's easier to do it from the back. Where to begin? First of all, to go to a planet that had never had an orbiter and to study the core and the geology and the topography and the composition of the surface materials and the exosphere, the magnetosphere, and the charged particle distribution, you need a pretty broad science team. You are doing almost everything in planetary science around that one planet. And that requires that collectively the science team have the breadth to do all that.

That said, there are different flavors of science team members. There are people who are good at getting into the planning for new data calibration issues, and data formatting issues, and archiving issues, and testing issues. And then there are co-investigators who are good at doing science analysis but aren't that interested in getting into the nitty-gritty of the data. A mission needs to have both. I think we did. Sometimes it's a little frustrating when the people who do the nitty-gritty want to also have time to get into the science analysis. That's a tension. I think on balance we have a good team.

Early on, because it had to be fairly big to have the scientific breadth, we imposed a structure to the team to give it a better ability to internally manage. So from the beginning we divided the team into four discipline groups, put people in charge of the discipline groups. Those groups would look after instruments or suites of instruments that addressed those disciplines.

We also have instrument scientists for each instrument. So there is somebody from the science team who is looking after the development of each of the instruments. What we didn't have, what the big missions have, is an instrument PI, someone who lives and breathes that instrument while that's being developed. And occasionally there were some times when we could have had somebody like that.

NIEBUR: Do you think that would have helped? Did instrument development ever suffer because of that? If they had a question, was there good communication back and forth between the instrument scientists and the developers?

SOLOMON: Generally, yes.

NIEBUR: That is a very difficult task.

SOLOMON: In the end most of the instrument scientists were, and probably had to be, at APL. We did have instruments that came in from other institutions. The laser altimeter came in from Goddard; the Mercury Atmospheric and Surface Composition Spectrometer came from the University of Colorado. Both of those were complete instruments that were delivered from outside partnering organizations. That worked pretty well. There was somebody at APL that followed the development of each instrument. But, really, the live-and-breathe guys were at the partnering institutions and not at APL.

Then we had a couple of instruments where the partnering was through pieces of instruments. It worked okay, but it wasn't perfect in terms of all the interfaces. The magnetometer was one, and the Energetic Particle and Plasma Spectrometer was another.

NIEBUR: That was actually the next question I was going to ask. With that many instruments, this was a huge mission in a tiny program.

SOLOMON: We still count seven instruments. But that's a lot. Several of the instruments have multiple sensors.

NIEBUR: Did you have someone in particular who is riding herd on the interfaces, or was that left more individually up to the teams and APL?

SOLOMON: A lot of the interface work was done by the instrument teams but the project science office did the herd writing. That was fine with me.

NIEBUR: Oh good, we are halfway through. Which is funny because we haven't done anything yet on the mission. We will talk for as long as you have time and when you are done, you're done.

So shortly after you were selected the Mars failures occurred [Mars Climate Orbiter, 1998 and Mars Polar Lander, 1999].

SOLOMON: Indeed.

NIEBUR: I mention it because it is important.

SOLOMON: It changed completely the attitude toward risk at Headquarters.

NIEBUR: And the requirements.

SOLOMON: The requirements changed. The review schedule changed. We did not begin to appreciate how that change was going to impact us until we were well along.

NIEBUR: I think that was generally true of the missions across the board. People did not expect NIAT [NASA Integrated Action Team] to have that kind of effect or cost.

SOLOMON: So we underestimated. They said, "How much do you need to respond to NIAT?" My project manager said, "We could do it for a million." Closer to 10, easily. That didn't help. But we did get some more money later on. Painfully, but we did.

NIEBUR: And when that came down, I remember the words "unfunded mandate," which were certainly valid. How does a team react to that kind of thing? Is it just, "thank you, we'll do it". I guess you didn't have much choice.

SOLOMON: We didn't have much choice. You say, "thank you you'll do it." What you say under your breath is something different. But you do it.

NIEBUR: Fair enough. Fair enough. So things changed.

SOLOMON: They changed the second time for us, with CONTOUR [Comet Nucleus Tour, 2002].

NIEBUR: Tell me about that. Tell me about how things changed when CONTOUR was lost. Since that was an APL mission as well, that had a direct effect.

SOLOMON: That was an APL mission, so that was different from simply saying, well, maybe faster, better, cheaper isn't quite what we want to do anymore. But now CONTOUR had overlap of some subsystems, overlap of some people. It was one of the few APL planetary missions.

NIEBUR: Did it require additional testing? Or were people just back-tracking paperwork. How did it affect your mission?

SOLOMON: Perpetual reviews. If you press me, I will tell you my opinions of reviews.

NIEBUR: I've heard some of your opinions.

SOLOMON: Some [reviews] are good. Some are good. But it put a lot of spotlight on APL, and so understandably there was more scrutiny of other missions in the pipeline at APL, including us.

NIEBUR: Is that right? Because that is not the way the mission line began. The mission line was supposed to be hands-off.

SOLOMON: But in the end I think the probable failure of CONTOUR had nothing to do with the design of our spacecraft, so it wasn't as bad as it might have been. Or as problematic.

NIEBUR: Good. So how did things change after confirmation? At confirmation, you guys had solved the problems. Did anything else come up in the site visit? You were rated low-risk before and low-risk after. Did anything change during the study?

SOLOMON: No, the site visit was a much more pleasant experience than the first site visit.

NIEBUR: So then you started working.

SOLOMON: We started working.

NIEBUR: And how long did things go smoothly?

SOLOMON: Through confirmation (laughing).

NIEBUR: I am trying to be diplomatic.

SOLOMON: Through confirmation.

NIEBUR: When did you know things were starting to go off the rails? I don't mean to be pejorative. I am trying to figure out how you know when something's in trouble. Later missions would say it was earned value. Earlier missions would say well, we didn't know. So I'm curious how you could tell that things started to go off the rails.

SOLOMON: An accumulation of signs. I'm trying to put the right year on everything. We had an 18-month Phase B. That's good. That gave us a lot of time to work out, on paper, all of the technical solutions we were going to need. Since we were selected in 1999, we started in 2000. Confirmation was June, I think, of 2001.

NIEBUR: March?

SOLOMON: Oh no, confirmation wasn't March. CDR [Critical Design Review] maybe was March.

NIEBUR: Oh, okay; I'm sorry.

SOLOMON: No I remember confirmation very well. Now Ed [Edward J.] Weiler is my best buddy. He says, "Remember, I'm the one who confirmed you." There was a time in between that and when we launched when we weren't best buddies.

NIEBUR: He was awfully proud at confirmation. At selection he was all over the media talking about how amazing it was you could pack this big mission into this little box.

SOLOMON: Yes. He also was selecting New Horizons at about the same time, talking about the bookends of the solar system. And now I am getting a little fuzzy on the year. There was a hit that I didn't appreciate early on. CONTOUR ran late. We were slow to ramp up to our planned staffing levels. So that was an early flag. We were underspending. Underspending looked good to me as the PI; we might have some money saved. No, it's bad, because it means you are not getting things done as quickly as you should, as I later came to appreciate more painfully. So that was perhaps the earliest flag, that our staffing—

NIEBUR: So it was kind of an earned value thing? Whether it was formal or not.

SOLOMON: Yes. We were slow to staff up. The big warning signs came, I want to get the year right, about 2 years into [Phase] C/D[1], when several things were happening. We started projecting staffing levels that were over the budget.

NIEBUR: To catch up?

SOLOMON: To solve technical problems. We were projecting delays at that point in key subsystem deliveries that came to pass. One of the most painful was the spacecraft structure.

NIEBUR: It was all APL built, right?

[1] It was a little more than one year into C/D (2002). The year was corrected later in the interview.

SOLOMON: No, no, no. That was subcontracted to an outfit called Composite Optics in California, because APL had never done a structure made out of composites. But we did it to keep the dry mass of the spacecraft down. Composite Optics is a fine company, but they're a small company, and the mission that they had to finish before us was MER [Mars Exploration Rover]. MER was 4 months late on the delivery of their spacecraft, the [bus] that flew the MER to Mars. And there was nothing we could do.

NIEBUR: No, you certainly couldn't get, a small mission in Discovery couldn't get, priority over something like MER.

SOLOMON: No, no. So that set our integration and test schedule 4 months in arrears from the beginning. Because the spacecraft structure had to go to the propulsion system guys, who integrated it. It was an integrated system. And then those guys delivered an integrated propulsion system and structure to APL. And then all of the rest of it went on at APL. So that put us deeper in the hole. But there were other things going on at the time. We were really sweating the inertial measurement unit [IMU].

NIEBUR: Oh, I remember the IMU discussions.

SOLOMON: You may know the story behind that. There was a company that built these things outside of Santa Barbara in Goleta, California. They were bought by Northrup Grumman. And Northrup Grumman decided to close the Goleta plant, and they tried to get people who knew

how to do this to move down to Woodland Hills. Well, nobody who lives in Santa Barbara wants to live in LA. So none of them moved. So they had to reproduce the expertise to build these very complicated gyros.

NIEBUR: Using all new people?

SOLOMON: All new people.

NIEBUR: Spending more time on training.

SOLOMON: They missed every deadline. And both Deep Impact and we were using virtually the same system. And we were both in danger of not having it in time to fly. So it's just another example of things that were bothering us. But there were other technical issues, and they were all getting translated into staffing months. And they were all eating away at our schedule.

So, it kind of came to a head at two spots. Max Peterson had the wisdom, after confirmation but before we had gotten to this point, to hire a deputy. What I didn't realize was that he was grooming his deputy to become MESSENGER project manager. And at some point Max told me, and now we are at late 2002, fall of 2002, that he was developing symptoms of emphysema. His doctor had told him, "Look, unless you want to die soon you have to quit smoking." The only way Max could be a project manager was to smoke. So he said, "I don't want to die. I am going to retire."

NIEBUR: I knew he retired for health reasons. I didn't realize it was that.

SOLOMON: So he could quit smoking and not die of some lung disease. So a transition plan was in place. The deputy was going to step up in December 2002, something like that, and run things. Well, I went out to a meeting we had with Northrup Grumman. It was on a Friday, I think, in November of 2002. The deputy project manager was there, and several APL guidance and control experts were there. We were talking with vice presidents and people like that. There were three or four vice presidents that were fired over the IMU.

NIEBUR: Wow, I didn't know that.

SOLOMON: Yeah, they just kept burning through managers because they couldn't solve the problems, they couldn't meet the specs. So we were having one of these meetings. The deputy project manager was there. The next Monday he didn't report for work. You probably know this already.

NIEBUR: I remember.

SOLOMON: Nobody heard from him. Finally Max, who treated this guy like a son, drove out to his house. He would barely talk to Max. The former deputy project manager couldn't sit down and explain what was going through his head. He literally stopped working that day. He never came back, never provided an explanation. He completely disappointed Max and left us without a deputy at a time when we were almost without a project manager.

NIEBUR: Right. And it wasn't a time when you could afford to be without a project manager. You were in the middle of I&T [Integration and Test].

SOLOMON: No, and we are falling behind on our schedule. And we are throwing staff months that we can't afford, to keep what little schedule reserve we had. So APL, to their credit, persuaded Dave [David G.] Grant to come off the TIMED [Thermosphere Ionosphere Mesosphere Energetics and Dynamics] program or at least to be project manager of both TIMED and MESSENGER to fix the MESSENGER project. But he was coming on in a situation where our ability to meet our schedule and cost were very much in question, and he didn't have much time to overlap with Max. Max stayed on another month or two, I don't remember which.

NIEBUR: It wasn't long, was it?

SOLOMON: It wasn't long. But he was available. And Dave had that amount of time to come up to speed and did a fine job doing that, given what he was handed. He quickly came to the conclusion that we couldn't meet both schedule and budget, and that's when we began to have a series of difficult meetings with [NASA] Headquarters. We had to wrestle with that question. If we were going to make it, we needed more resources.

The transition of project managers was over that change in year, 2002-2003.

NIEBUR: That makes sense to me because I was just coming in the program at that point and I was trying to learn what was going on and I couldn't keep track of who your project manager was.

SOLOMON: So for the first difficult meetings we had with Headquarters, Max was still there. They were in January 2003. Dave Grant was already on board. We realized we needed additional resources. Still, we were working toward a schedule that would have had us go in our first launch window, which was March of 2004. There was another window in May of 2004. There was a third, less desirable window in August of 2004. So we had three windows, by good fortune, in 2004.

NIEBUR: Well, maybe good fortune.

SOLOMON: Maybe good fortune.

NIEBUR: It saved you from getting terminated, but on the other hand whenever you do a schedule slip there is a lot of cost involved.

SOLOMON: That's right. That's right.

NIEBUR: So, were you at all hesitant to push for a delay?

SOLOMON: Of course. We didn't. It was very uncomfortable to be in a position to ask for more resources. It wasn't a pleasant response at NASA Headquarters. And so there were lots of incentives to try to solve the problem without asking for more resources. We particularly didn't want to have the August launch, because that was the energetically least favorable launch. The

March and May launches involved cruise times of five years. The August launch, which is the one we eventually used, was a 6 ½-year cruise. And so not only would we get to the planet much later, but there would be a big Phase E cost increase. So we didn't want to go there. We wanted to get to Mercury sooner. So, in the winter of 2003 we were still aiming for the March 2004 launch. We then had the schedule reserves to do that. But it was APL's judgment, which I concurred with, that we were exceeding the cost cap. We would use up all our reserves and then still be in some jeopardy.

NIEBUR: So you had no choice.

SOLOMON: We had no choice. That's right.

NIEBUR: You came to Headquarters and began having the discussions. We mentioned, you alluded earlier to all the reviews. Did that trigger a series of reviews, or was that handled more internally?

SOLOMON: Oh yes. No, well, we had meetings that became reviews. I never sat down to write all the reviews we had. But we had many. There were definitely cost reviews that were triggered by our visits to Headquarters.

NIEBUR: In fact, at least one was a termination review, was it not?

SOLOMON: It was never called that.

NIEBUR: It was never called that?

SOLOMON: Ahead of time.

NIEBUR: I'm sorry. It is terrible to talk about these things.

SOLOMON: There was one meeting we had when we thought we were talking about cost that became a cost review. We were asked to do something that we had never heard before called a Risk Retirement Review. That was right before this cost review.

NIEBUR: I think that had been borrowed from another program because it was a [Kenneth W.] Ledbetter kind of thing.

SOLOMON: To be honest, many of the reviews are a blur. We had some high-level reviews in part triggered by this cost growth in which we had really distinguished former center directors and people like that come in. Worthless reviews. Totally worthless reviews.

NIEBUR: Because they only have the information that you give them?

SOLOMON: Because they only have the information that we give them. And so, surprise, surprise, at the end of the day they say, "Here are the problems we see." And they are the ones we pointed out. So we say, "Hmm, thank you. You validated our opinions." But a whole day is

gone, for people who should be working. The most valuable reviews by far were the kind of table-top subsystem-level reviews, where we brought in really expert people who worried about, say, thermal designs. And it was informal, and it wasn't wasteful. Those reviews are really valid. And the other valuable kind of review we had involved really smart people who came to multiple reviews. One of the standouts was Steve [Steven J.] Battel.

NIEBUR: Oh, Steve is wonderful.

SOLOMON: He is wonderful. He was on our site visits before we were selected and agreed, because he thought what we were doing was challenging and interesting, to be on a lot of our reviews after that. So he followed us from when we were a glint in NASA's eye to getting serious toward launch and was a steady source of really good ideas.

NIEBUR: Well, that's great. And that was on the project's initiative, or was he on the IAT [Independent Assessment Team]? Because you had an IAT that was assigned to you at selection, correct?

SOLOMON: We did. We did.

NIEBUR: But that was kind of the thinking. That you would have about the same people go through the life cycle of the projects so you wouldn't have to waste so much time educating new folks. Did you feel that was helpful at all?

SOLOMON: That was helpful. That was helpful, until they lost credibility with Headquarters. Headquarters thought the IAT weren't independent enough. When we were at some of our most confrontational meetings with Headquarters, as you may know, it was a time when there was not a lot of trust between Headquarters management and the Discovery Program Office [which at the time was within the NASA Management Office, located at JPL]. The Discovery Program Office was the organization that had appointed our IAT.

NIEBUR: That's when it was still at JPL. As I recall they were very involved with the preparation of your reviews.

SOLOMON: Yes, yes. They were extraordinarily helpful, to be honest. They were very helpful both in finding us good people to advise us and helping us identify problems, helping us identify solutions. So we had a wonderful relationship with Dave Jarrett. But Dave Jarrett was just at loggerheads with some key people at NASA Headquarters, so there was crossfire there with us in the middle. So our IAT was kind of a victim of that. They were viewed as a vehicle for the Discovery Program Office and therefore not independent of the Discovery Program Office. To the extent that there was a loss of trust between the Discovery Program Office and NASA management, the IAT became less effective. They were great for us. And I think you are right; Steve was on that. I would have to double check.

NIEBUR: Probably, because he was always funded through the IATs, and he shows up at a lot at reviews. And I know we were trying very hard to keep the membership consistent. Oh dear,

speaking of going off the rails, we've gone through all the PM stuff. You had two project scientists? Was Robert Gold also a project scientist?

SOLOMON: No, never. He was involved from the very beginning. Helped us write the first proposals and concept studies. But his role was always science payload manager.

NIEBUR: Payload manager. That's right, I apologize.

SOLOMON: And he has kept that role. I have had lots of project managers but only one project scientist.

NIEBUR: Tell me all the project managers you have had.

SOLOMON: We had the first one for the first proposal and concept study. He left. Then Max Peterson, and his doctor made him leave. Then Dave Grant. Dave kind of fell on his sword for us a couple years ago.

NIEBUR: This was after I left. I suppose I don't know about this.

SOLOMON: We had the need for additional personnel in mission operations and science operations. We needed a bit more money, and what drove home the point and caught NASA's attention was that there was a string of about nine months where there was a series of noticeable, not fatal, anomalies in the spacecraft, many of which could be attributed to people not investing

enough time in double checking commands for transmissions. So the judgment was that our operations teams were too thinly manned, that we needed a little more depth so that we could reduce human error on communications to the spacecraft and on planning and all the strategic things to a low-risk level. But integrated over the duration of our mission, I don't remember the number, but it was several millions of dollars extra that we needed. It was in the end granted, but it cast Dave into a poor light. The perception was that he could have foreseen this and better managed his resources. APL management made the call that he should pass the reins on.

NIEBUR: Interesting. You seem sad.

SOLOMON: Yeah, I wish it hadn't happened. Or if it had to happen, I wish it had happened at a time that was more natural for his own career. But we had at that point a deputy project manager, Peter [D.] Bedini. A wonderful project manager. He was able to take the reins smoothly from Dave. It's just that Dave was put out to pasture by his own management before he had made that decision.

NIEBUR: That is unfortunate. Well, my next bit is about launch, which is a happier bit. But before we get there, looking back at that time, during I&T when there was so much trouble. When it became, I am going to assume it became all-consuming. I am just going to project here and assume that's it. Looking back on it, what would you say it was? Was it the failure of the IMU to come in on time? The spacecraft to come in on time? Or was there more general underlying cause? Because cost overruns are just the symptom that something that has gone wrong.

SOLOMON: There were three management positions at the subsystem level, well two at the subsystem level and one at I&T, that had the wrong individual.

NIEBUR: So it comes down to people?

SOLOMON: It comes down to people.

NIEBUR: In terms of experience or skills?

SOLOMON: Now that is not the whole story.

NIEBUR: Did they not have enough experience or were they not the right kind of person for the job?

SOLOMON: In two cases they were internal APL people who were being, well, at least in one case there was one guy who was given more responsibility than he had had before. But he wasn't meeting deadlines. In another case it was somebody who was really good in theory but in terms of delivery it just didn't happen. They were kept on, in my view, which I made known, longer than they should have. All three of these guys were eventually replaced. But the two APL, long-term APL, guys were replaced rather late, and one was mission operations and one was in autonomy. We had a lot of unresolved autonomy issues at the time we did our pre-ship review.

NIEBUR: Wow, that late even?

SOLOMON: Yes. Now, we were shipping to meet the second launch window, which we didn't make. The I&T lead was really a different story. It was somebody fairly new to APL. I think his background was Goddard, but he had a very different style and way of doing business from APL. And there was a lot of friction between him and the system engineer. And it just led to unhappy people and, I suspect, delays.

NIEBUR: But that kind of thing can have that kind of an impact on a mission?

SOLOMON: Oh yes. That is such a critical path. That's critical path now. All the integration and testing, it's critical path. So if you don' t have a cohesive organization running that ship, you are not going to stay on schedule.

NIEBUR: And time is money.

SOLOMON: Yes. But all kinds of things were going on then. There was, it was, a struggle. I didn't even see most of it. But there was a struggle between this I&T lead, the project manager, and the mission system engineer about how to run I&T. And the I&T lead was really unhappy and developed a few colleagues who were really unhappy with the way management was going. And then he did something I think was inexcusable. He started calling a colleague at Headquarters and saying this mission is at risk. The person at NASA Headquarters, who I am not going to name—

NIEBUR: I am not asking.

SOLOMON: Was a former APL person. So there was a communication on that. But that went all the way to the top of the building. That kind of information went all the way to the top of the building. So we had attention by Ledbetters and [Christopher J.] Scoleses that normally, first of all, was uncalled for. It wasn't true. And secondly it was way out of proportion to the kind of attention that one normally gets.

NIEBUR: And all that happened because one lead was disgruntled or not in step?

SOLOMON: He wasn't getting the autonomy, the ability to run the I&T effort, that he thought he should have on the basis of how he felt I&T should run. Whereas the project manager was saying, look, I'm the manager, this is how it's going to run.

NIEBUR: That is a difficult problem to solve because normally you would.

SOLOMON: No, it's easy to solve [gesturing].

NIEBUR: It was?

SOLOMON: Yes, but they didn't do that. Even I saw that problem. They did it, but they didn't do it quickly enough. They did do it. They got rid of him. They got rid of all three of these guys and replaced them with people who were much better. But it cost us a lot of time.

NIEBUR: That is an important lesson though. That it is really important to look at the composition of your team and certainly a PI coming in from the outside doesn't have that kind of insight to begin with. But the management institution has, and has to be careful, are you putting the first string on this mission. So take us to happier times. Take us to launch.

SOLOMON: Okay.

NIEBUR: So, you're there. You are getting ready to launch. Was your science team there? Were the engineers there? Can you describe it?

SOLOMON: Yes. Yes.

NIEBUR: Give us a happy story. People were tense? People were excited? People couldn't believe we were actually going to Mercury?

SOLOMON: No, no, people were excited. We really thought as a group we could have launched in the second window. The Solar System Exploration Division Director told us no, you are not going to launch in that window, you are launching in August. So we had extra months of testing. We did testing, particularly on the autonomy, that we wouldn't have otherwise done. We had

guys who were spending a month or two at Astrotech playing with the spacecraft. We felt really ready to launch at that point. More than ready.

NIEBUR: Where did you watch the launch?

SOLOMON: Oh, I was at the one of the so-called VIP viewing areas. I had my wife and three grandchildren there. The launch was a little after 2 a.m., and the grandkids were young.

NIEBUR: Oh, how young?

SOLOMON: In 2004, so that's 5 years ago, they were 6, 4 and 3.

NIEBUR: They must have been thrilled to be out here with Grandpa.

SOLOMON: Oh yeah, they were. They had a great time. We launched on the second day of an almost 3-week window. A tropical storm, vestiges of a tropical storm, had stopped us the day before. The day didn't satisfy the constraints on clouds, but we came very close. We came within a few minutes of liftoff. We were out there at night, watching. And then the next night everything was go. Which was good, because another storm came through a day or two later that turned into a hurricane.

NIEBUR: Oh boy.

SOLOMON: This is August.

NIEBUR: Not a good time to be launching from Florida.

SOLOMON: No. We had named storms right in front of us and right after us and just this one-night window when we could go. And we went.

NIEBUR: Wow, excellent.

SOLOMON: It was a beautiful launch. A Delta 2 heavy with the 9 solid boosters, 3 of them ignite in the air. So it was really gorgeous.

NIEBUR: Oh I bet. It must have felt a perfect feeling to get that off the ground, safely in the air shall I say.

SOLOMON: I can't say I was not tense up until that launch. It was, at that point, it was completely out of my control. And yet I realize that a launch is a complicated operation. Of course many people who know much more about launches are making things happen. But we had done the best to deliver our spacecraft. Now we are dependent on Boeing and folks at KSC [Kennedy Space Center]. They came through, so that was good.

NIEBUR: I only have one more set of questions and that's about the flybys. Now when you were postponed to August, your flybys changed. You lost your Earth flyby, right?

SOLOMON: No, we gained. We gained.

NIEBUR: You gained an Earth flyby.

SOLOMON: That's right.

NIEBUR: Now, for the first Venus flyby you were on the other side of Venus. You didn't really get any data.

SOLOMON: That is right. Venus was at superior conjunction. We actually had a safe hold the day before; we lost communication. We flew by Venus at very high altitudes, 3,000 km. No operations were planned, but we were on safe hold during the whole thing. It was a little interesting.

NIEBUR: I would say.

SOLOMON: The second Venus flyby was June of 2007, and that was a much closer approach. We needed that close approach because we not only slowed the spacecraft down, but we changed the plane of the orbit. Venus is almost in the same orbital plane as the Earth. Mercury's orbital plane is inclined a little more than 7 degrees to the ecliptic. So it was that Venus flyby that put us in Mercury's orbital plane instead of the Earth/Venus plane.

NIEBUR: But you made good use of your time there. You turned on how many instruments?

SOLOMON: All of them. We wanted to practice the Mercury flybys. We wanted to practice the types of commands we would use. We wanted to turn on every instrument even though some of them didn't have a good prospect for novel science to come out of observations of Venus. But we wanted to go through the exercise that we would do seven months later at Mercury and do a dress rehearsal of the whole payload.

NIEBUR: How did it come out? For the record.

SOLOMON: Beautifully. Beautifully. Virtually everything that we had programmed in worked. We learned a few things about some of the sequencing on the imaging system that we put to good use on Mercury. But all the instruments worked well. We actually got some science out of the flyby because we worked with the Venus Express [European Space Agency mission] guys who of course were in orbit around Venus at the time we flew by. But they were at a very different position with respect to the Venus atmosphere and induced magnetosphere. And so the combination of measurements that they and we can do has made for some interesting science that is now appearing in papers that the two teams have been collaborating on.

NIEBUR: Excellent. And the teams themselves have been working together.

SOLOMON: That's right.

NIEBUR: How do you start a collaboration like that? Did you know the PI of Venus Express?

SOLOMON: No, not particularly well. No, no. So I got to meet the project scientist of Venus Express and some of the key players. We started some sessions, meeting at international meetings, before we got there. We said we are flying by while you are there. We have instruments that complement yours. What do you think about sharing data and doing some collaborations? That got down to the level of PIs on individual experiments on Venus Express collaborating with their counterparts on MESSENGER.

NIEBUR: Nicely done. Excellent. Excellent.

SOLOMON: Yeah. That all worked very nicely. So that was fun. In some ways the Mercury encounters came too soon because some of the collaborative Venus stuff is not quite finished. Because we suddenly turned to Mercury, seven months later. But those papers are now starting to come out, the Venus papers.

NIEBUR: In fact one of the things, I think I mentioned this to you at DPS [American Astronomical Society's Division for Planetary Sciences meeting], is that I have been very impressed with MESSENGER in that you have had papers come out early and often. And I think that has been an amazing science return already and you haven't even got there to orbiting yet.

SOLOMON: We are going to be really busy when we are in orbit. So having these flybys has benefitted us in many, many ways. The first is that we are getting new science from each of the flybys. We are taking instruments, many of which weren't around at the time of Mariner 10, and turning them on Mercury for the first time. So there are lots of new things to see. We are using the flybys to help us understand the instruments, and they are feeding back into calibrations, to operational sequencing ideas as we develop all of the plans for the orbital phase of the mission. And there everything will be happening so fast we have to plan and nearly fully program the spacecraft for that operational phase ahead of time. We have some ability to target, and we are building up a long list of targets from the flybys.

NIEBUR: Wow, you've already got that?

SOLOMON: Yes. It is over 1,500 targets that we have so far.

NIEBUR: How long will it take you to observe 1,500 targets? I know that the instruments are different.

SOLOMON: Yeah, the instruments are different.

NIEBUR: How long will that take you?

SOLOMON: I don't know. Most of the targets are for the imaging system, but we have things we want to make sure we have an altimetric track over, lots of spectroscopic targets. And individual

observations are matters of second. In a year it is not a big fraction. But we knew we would have some portion of every orbit devoted to targeted observations. And now we just have to marry the specific targets against the opportunity of time we have and the mission time.

NIEBUR: That is going to be a lot of data.

SOLOMON: Yes. Of course.

NIEBUR: It's exciting. It's thrilling that there is going to be that much data.

SOLOMON: Each one of our flybys has been a tremendous source of new scientific information. We are still learning many things from the two flybys that we have had so far. When we are in orbit, we will have two orbits a day. And this is a 12-hour orbit. And so every day we are going to go through what we went through for all of 2008.

NIEBUR: Do you have any strategies for drinking from the fire hose like that?

SOLOMON: Yes, we are developing them. But I think we will be somewhat surprised by the intensity and continuity of that exercise compared to the flyby experience.

NIEBUR: It's a good problem to have.

SOLOMON: That is a very good problem to have. So that is the problem with getting out the science. I think we were lucky that the flybys were spaced far apart so that we could do a lot of science and finish papers.

NIEBUR: Sure, people get their tools from it as well.

SOLOMON: Yes, that's right. That's right.

NIEBUR: So hopefully it will go smoother. Well, that is all very exciting. I am not going to ask you about the science results because I know you have a whole morning session on it on Tuesday.

SOLOMON: Right.

NIEBUR: I am looking forward to hearing that. So I guess as we wrap it up, one of the reasons that we are doing this study is to look at what kinds of things have worked, where things have gone more difficult, and how to improve things for later on. If you could go back and talk to a younger Sean, what would you tell him? What would you tell people who are looking to do planetary science missions in the future?

SOLOMON: I am not quite sure how to answer that question.

NIEBUR: It's an open question.

SOLOMON: I learned an awful lot in the course of being a PI about engineering, subsystem engineering, systems engineering, about project management. I don't know any way to learn that ahead of time without being involved at some level. And I clearly made some mistakes not knowing some of these things better. We were really knocked around quite a bit, I think, in ways that a PI now would not be because of all the changes to the Discovery Program. You know between the time we were selected and the time we launched there were, I counted it once, five different management arrangements for the Discovery Program.

NIEBUR: That's right. It was in flux.

SOLOMON: Constant flux. And some of the solutions were really terrible. When we started out it seemed stable. The Discovery Program Office was out of the NASA Management Office at JPL. They were understaffed, but they had a stable set of people. And then when that broke down and NASA had to find a way to manage the program, they tried to bring in private industry, and then they tried to have JPL itself manage it, which was very awkward because of the conflict of interest on a competing program line like Discovery. And then they threw additional levels of management at it at Headquarters. None of those were stable for more than a few months.

We were busy going through the most difficult time of Phase C/D, while all that was happening. We were suddenly having to deal with new people, new concerns, and new priorities. Every time there was a change, the whole review structure changed. New types of reviews started. When there was a management change at NASA Headquarters, we went

through multiple directors of the Planetary Exploration Division. Every time there was a new one, okay, new sets of reviews. Different issues that are at the forefront of the new management.

NIEBUR: That's true. In 2003-2004 alone there was Colleen [Hartman] and Orlando [Figueroa] and Andy. He went very quickly too, didn't he?

SOLOMON: Exactly. And every one of them changed what was important.

NIEBUR: That's right.

SOLOMON: And we had to change to be consistent with what was important at Headquarters. And trying to keep on schedule and budget – that was the most difficult time in the mission, by far. It would have been challenging enough without all of the management changes from above. But it was nearly intolerable with all those. And some managers were helpful, and some didn't help that much.

NIEBUR: I'm sorry the mission had to go through that.

SOLOMON: All I can say is that since we launched, having the Discovery Program Office at Marshall [Space Flight Center, Huntsville, Alabama] has been a breath of fresh air.

NIEBUR: Really?

SOLOMON: They have been stable. The people have been helpful and knowledgeable. They stay involved, but they don't micromanage. And there is a continuity of personnel and thinking. So the last 4 ½ years have been terrific. To contrast that to the 2 years right before that is like night and day.

NIEBUR: It makes a big difference then, is what you're saying.

SOLOMON: Yeah.

NIEBUR: Good lessons. Well, thank you, Sean. I very much appreciate your time with me. This has been absolutely great, and I will send you a copy of the transcript when it is done.

[End of Interview]